ON THE LATENT HEAT OF EXPANSION IN CONNECTION WITH THE LUMINOSITY OF METEORS, ETC.

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In 1863, in a paper published in Silliman's Journal (vol. xxvi. p 92), I attempted to show that the luminosity of meteors is probably due to the effects of latent heat.

An abstract of my results was made by the "Luminous Meteor" Committee of the British Association, and published in 1864 with their report. The paper was also favorably referred to by Haidinger ;* but it evidently has not been accepted as furnishing a satisfactory solution of the problem.

My explanation was based upon the assumption that when air is heated "under a constant pressure," the heat required to produce a given elevation of temperature, in excess of that required to produce the same change of temperature "under a constant volume," remains latent in the expanded air. But according to the dynamical theory of heat, the most of this excess is employed in lifting the weight of the atmosphere ; a glance, however, at the tabular statement in my paper, shows that a very small fraction only of this excess is actually required to produce all the effects which I attributed to it. Hence, although "latent heat of expansion" seems to be generally ignored, I have always—in view of the remarkable correspondence of the observed phenomena with what might be expected to occur, supposing my explanation to be correct—felt an assurance that it must have some foundation in truth. The fireballs of December 24th and January 2d having prompted me to re-examine the question, I find this impression strongly confirmed, and therefore venture again to call attention to the subject, hoping to make it appear probable, not only that "latent heat of expansion" is a reality, but that it plays a leading part in all the luminous phenomena of the upper regions of the atmosphere.

Tyndall, in his treatise on "Heat as a Mode of Motion" (New York,

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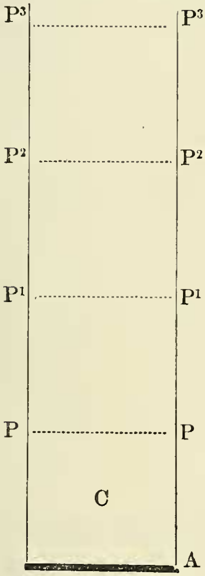
* Memoire sur les relations qui existent entre les ètoiles filantes, les bolides, et les essaims des météorites ; par M. Haidinger, Associé de l'Académie — Bulletins de l'Academie Royale de Bruxelles, 2e series, T. XVII., 1864. p. 133 "M. Quetelet, dans son important ouvrage sur la *Physique du Globe*, publié en 1861 (p. 5), désigne ces couches par les noms *d'atmosphere mobile* ou *dynamique*, et *d'atmosphere immobile* ou *stable*. Les considerations publieés par M. Benjamin V. Marsh dans le journal americain du Professeur Silliman ont grande importance relativement à l'existance de deux couches atmosphèriques de nature différente."

1863, p. 83), says: "Let C be a cylindrical vessel, with a base one square foot in area. Let P P mark the upper surface of a cubic foot of air at a temperature of 32° F. The height A P will then be a foot. Let the air be heated till the volume is doubled. To effect this it must, as before explained, be raised 490° F. in temperature, and, when expanded, its upper surface will stand at P^1 P^1, one foot above its initial position. But in rising from P P to P^1 P^1, it has forced back the atmosphere, which exerts a pressure of 15 lbs. on every square inch of its upper surface; in other words, it has lifted a weight of 144 × 15 = 2,160 lbs. to a height of one foot.



The capacity for heat of the air thus expanding is 0.24; water being unity. The weight of our cubic foot of air is 1.29 oz., hence the quantity of heat required to raise 1.29 oz. of air 490° F. would raise a little less than one fourth of that weight of water 490°. The exact quantity of water equivalent to our 1·29 oz. of air is 1.29 × 0.24 = 0.31 oz. But 0.31 oz. of water heated to 490° is equal to 152 oz., or 9½ lbs heated 1°. Thus the heat imparted to our cubic foot of air, in order to double its volume and enable it to lift a weight of 2,160 lbs. one foot high, would be competent to raise 9½ lbs. of water one degree in temperature.

The air has been heated *under a constant pressure*, and we have learned that the quantity of heat necessary to raise the temperature of a gas under constant pressure a certain number of degrees, is to that required to raise the gas to the same temperature *when its volume is kept constant*, in proportion of 1.42 : 1; hence we have the statement 1.42 : 1=9.5 lbs. : 6.7 lbs., which shows that the quantity of heat necessary to augment the temperature of one cubic foot of air, at a constant volume, 490°, would heat 6.7 lbs. of water one degree.

Deducting 6.7 lbs. from 9.5 lbs., we find that the excess of heat imparted to the air, in the case when it is permitted to expand, is competent to raise 2.8 lbs. of water one degree in temperature.

As explained already, this excess is employed to lift the weight of 2,160 lbs. one foot high. Dividing 2,160 lbs. by 2.8, we find that a quantity of heat sufficient to raise one pound of water one degree F. in temperature, is competent to raise a weight of 771.4 lbs. a foot high.

This method of calculating the mechanical equivalent of heat was followed by Dr. Mayer, a physician of Heilbron, Germany, in the spring of 1842."

Now, since equal additions of heat make equal additions of volume, this process of heating and expanding might be continued indefinitely, with like results. That is to say, P^1, P^2, P^3, &c., being at intervals of one foot, the upper surface of the air will stand at P^2 when the temperature has risen twice 490°; at P^3 when it has risen three times 490° F., and so on; one volume being added for each rise of 490° in temperature, and the expenditure of heat being the same for each.

If we take for our unit, the heat required to raise the temperature of one volume of air 1° under constant volume, the total expenditure of heat whilst one volume is added to the bulk, will be $490° \times 1.4 = 686°$; and the heat expended, in excess of that required to produce the elevation of temperature alone, will be $686° - 490° = 196°$.

This expenditure has enabled the air to accomplish two results; to lift 2160 lbs. one foot high, and to fill an additional volume. Prof. Tyndall assumes that the space-filling was accomplished without the expenditure of any force whatever, and that the whole 196° were employed in lifting the weight. But, inasmuch as this may be considered an open question, we will take x to represent the heat, if any, employed in producing and maintaining the change of bulk; that is to say, the "latent heat of expansion;" and proceed to consider what must be the relation of latent heat to volume, independently of any particular value of x.

Since both the expenditure of heat, and the weight lifted, are precisely the same, during the addition of each volume, the remainder,—represented by x—must also be the same. Hence, when one volume has expanded so as to fill

2 vols.—these contain x degrees of latent heat, the No. in each being $\frac{1}{2}$x
3 " " 2x " " " " $\frac{2}{3}$x
4 " " 3x " " " " $\frac{3}{4}$x
100 " " 99x " " " " $\frac{99}{100}$x
and so on.

Whence it appears that the less the density of the air, the greater will be the amount of latent heat, in a given volume; although, for air of considerable rarity, the change is so slight that the latent heat per volume may be considered as sensibly constant. We have treated only of the heat rendered latent during the expansion of air of standard density, which must already contain latent heat. If, however, we start from the liquid condition of air, a similar train of reasoning leads to the conclusion, that the total amount of latent heat per volume is absolutely the same for air of all densities. It must also be the same for all temperatures. For if, when the surface of the air is at P^1 we suppose the top of the vessel to be prevented from moving, and the whole to be cooled down until the temperature of the air returns to 32°—the specific heat under constant volume being the same for all temperatures—the heat given out during each degree of cooling will be the same; being exactly equal to that which, under constant volume, would be required to raise the temperature one degree. Consequently, the latent heat must remain unchanged during the process. In other words, the latent heat is independent of temperature, and is therefore the same per volume for air of any given density, whatever may be its temperature or previous history.

Hence, as this heat represents the force which is employed in maintaining the volume of the air, and as its amount depends upon the volume alone, it may perhaps more properly be termed the "latent heat of volume"—or briefly, the "*volume heat*" of air.

It is evident that we may readily determine, in terms of x, the amount of latent heat (in excess of that rendered latent between the liquid condition and the standard pressure) contained in a given volume, or in a given weight, of the atmosphere at any height. The known law of variation of density is such that at the height of 3.43 miles the density is half that at the level of the sea—at twice that height, $\frac{1}{4}$—at three times, $\frac{1}{8}$, and so on—the density diminishing in a geometric, as the height increases in an arithmetic ratio. Whence we see that one volume at sea-level, at the height of

3.43 miles	becomes	2 vols.—	containing	x deg. of latent heat	the No. in each vol. being	$\frac{1}{2}$x
6.86	“	4 “	“	3x “	“	“ $\frac{3}{4}$x
10.29	“	8 “	“	7x “	“	“ $\frac{7}{8}$x
34.30	“	1024 “	“	1023x “	“	“ $\frac{1023}{1024}$x
68.60	“	1,048,576	“	1,048,575x	“	$\frac{1,048,575}{1,048,576}$x

and so on.

The volume, and consequently the latent heat, of a given weight of air being doubled by each addition of 3.43 miles to the height, it is evident that each molecule of air, near the upper limits of the atmosphere, has, associated with it, an enormous amount of latent heat. But this need not excite great surprise : for when we consider that $\frac{1}{3}$ of a grain of air at the surface of the earth occupies only one cubic inch, whilst at the height of one hundred miles the same occupies one thousand millions of cubic inches, every part filled completely and equably, each molecule being held in its place; at a certain definite distance from its fellows, we cannot doubt that it has abundant use for all its stores of energy, in constructing and maintaining the framework of this vast edifice ; unless, indeed, we conclude that space-filling is a kind of work which—unlike every other—does itself.

We may form some idea of the value of x, by comparing the heat expended with the work done, in the experiment already quoted from Tyndall. He shows that the expediture of heat, in excess of that required to raise the temperature under constant volume, is competent to raise 1° the temperature of 2.8 lbs. of water. If we take 772 foot-pounds as the mechanical equivalent, the same would be competent to raise 2.8 × 772 = 2161.6 lbs. one foot high, showing an excess of 1.6 lbs. over the weight actually lifted. The amount of heat applicable to the work of expansion or space-filling, for this value of the equivalent, is therefore very small. But there is some evidence in favor of a larger value of Joule's equivalent, and, consequently, in favor of a larger value of x.

In his final paper,* Joule announces the following results from his experiments:

From	1st	Series,	consisting of	40	experiments from friction of	water, value of equivalent.	772.692
“	2	“	“	20	“	mercury “	772,814
“	3	“	“	30	“	“ “	775.352
“	4	“	“	10	“	cast-iron “	776.045
“	5	“	“	10	“	“ “	773.930

Whence we see that the mean result from the whole 110 experiments was.. 773.857

Joule adds, "I consider that 772.692, the equivalent derived from the friction of water, is the most correct, both on account of the number of experiments tried, and the great capacity of the apparatus for heat; and since, even in the friction of fluids, it was impossible entirely to avoid vibration, and the production of a slight sound, it is probable that the above number is slightly in excess;" and he concludes by adopting 772 as the most probable value.

Now, inasmuch as, in the case of cast-iron, he had made an experimental determination of the heat expended in the production of sound, and had allowed for it; and since no further explanation is given, we must look upon his final conclusion as based upon the 40 experiments with water alone; the fractional part being rejected in consideration of probable loss from the noise produced in that series of experiments.

Although Joule thus ignored nearly two-thirds of a series of experiments, all of which had been conducted with equal care, and each of which would therefore seem to be entitled to some weight, he placed their results upon record; and we may certainly be permitted to inquire what consequences would have followed the adoption of the purely experimental value which the whole series indicated—773.857.

Adopting this as the mechanical equivalent of heat, we find the force competent to lift $2.8 \times 773.857 = 2166.8$ lbs. one foot. But the weight actually lifted was 2160.0 lbs. one foot. Hence, a force competent to lift 6.8 lbs. one foot has become latent, having been employed in producing and maintaining the expansion; but, inasmuch as the quantity of heat necessary to augment the temperature of one cubic foot of air (weighing 1.29 oz.) at a constant volume, 490°, would heat 6.7 lbs. of water 1°, it would be competent to lift $6.7 \times 773.857 = 5184.8$ lbs. one foot high; whence we have the statement:

Foot-pounds. Foot-pounds.
as $5184.8 : 6.8 :: 490^\circ : 0.642^\circ = x$, showing that sufficient heat was rendered latent, to raise the temperature of the whole mass of air (1.29 oz.) at a constant volume, 0.642 degrees.

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* Philosophical Transactions 1850.

Owing to the uncertainty as to the exact ratio of the two specific heats of air, and as to the exact value of the mechanical equivalent of heat, an accurate determination of the value of x cannot yet be reached; but since the above value is based upon the complete series of experiments made by Joule in 1849, it must be entitled to consideration as a first approximation, and may be used to illustrate the action of latent heat in the production of luminous phenomena.

But we are here met by the assertion of several standard writers, that the existence of latent heat of expansion was positively disproved by a certain experiment performed by Joule, who announced in the Philosophical Magazine for May, 1845, that "no change of temperature occurs when air is allowed to expand in such a manner as not to develop mechanical power."

Although the interpretation thus put upon Joule's words seems to be perfectly natural and legitimate, an examination of the memoir in which he describes his experiments, and announces this conclusion, seems to show that he did not intend them to be so interpreted.

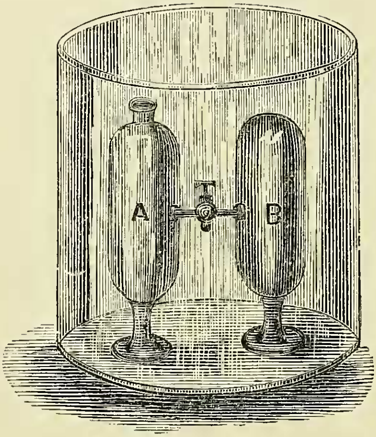
Prof. Balfour Stewart, in his "Treatise on Heat" (1871, p. 317) says: "Many familiar experiments show that when a gas is suddenly compressed, there is a production of heat, and that when suddenly expanded there is an absorption of heat.

Séguin and Mayer had already suggested the use of gases and vapors for the purpose of determining the mechanical equivalent of heat; and air, the substance chosen by Mayer, was no doubt very good for such a purpose; nevertheless, the suggestions of these philosophers do not seem to have been accompanied with a clear appreciation of all the data necessary to a complete proof.

Joule, however, in his experiments, supplied what was wanting in order to derive a good determination of the mechanical equivalent of heat from the known gaseous laws. By compressing air forcibly into a receiver surrounded by water he found that the water was considerably heated. It is not, however, correct to infer without further experiment that the amount of heat produced in this case is the exact equivalent of the energy expended in compressing the air. A familiar instance will make this clear. By a blow of a hammer upon a small quantity of fulminating mercury, it is exploded, and produces a considerable amount of heated gas, but we are not at liberty to suppose that all the heat thus developed is merely the mechanical equivalent of the energy of the blow, as will be evident by supposing such an extreme case as a ton of fulminating powder.

Evidently the substance is in a different molecular condition at the end of the experiment and at the beginning, and it may be supposed with much truth that the heat produced is nearly all due to the conversion into a kinetic form of a certain potential energy present in the compound. Now in the experiment above described, in which air is compressed, the air is evidently in a different molecular condition after compression, for the particles are much nearer together. The first thing, therefore, is to determine how much, if any, of the heat produced may be due to this change of the molecular condition of the air, and how much to the work expended in compressing the air.

The following very ingenious experiment performed by Joule is conclusive in showing that the mere change of distance of the molecules of a permanent gas neither produces nor absorbs heat to an appreciable extent. In the figure, we have two strong vessels, of which A contains compressed air, say under the pressure of 20 atmospheres ; B, on the other hand, is a vacuum. The two vessels are connected with each other by a tube having a stop-cock, which we may suppose to be shut. The whole apparatus is plunged into a vessel of water. After the temperature of the water has been very accurately ascertained, open the stop-cock, and thus allow both vessels to have the same pressure.



When the experiment is finished it will be found that there is no change in the temperature of the water. The prevalent idea is that when air expands it becomes colder, and that when condensed it becomes hotter ; but Joule, by this experiment, has shown that no appreciable change of temperature occurs when air is allowed to expand in such a manner as not to develop mechanical power."

Prof. Tyndall ("Heat considered as a Mode of Motion"—1863, p. 88,) in introducing this experiment, says : "Is it not possible to allow a gas to expand, without performing work? This question is answered by the following important experiment, which was first made by Gay-Lussac," and, after describing it, he says "We are taught by this experiment that mere rarefaction is not of itself sufficient to produce a lowering of the mean temperature of a mass of air. It was, and is still, a current notion, that the mere expansion of a gas produced refrigeration, no matter how that expansion was effected. The coldness of the higher atmospheric regions was accounted for by reference to the expansion of the air. It was thought that what we have called the "capacity for heat" was greater in the case of rarefied than of unrarefied gas. But the refrigeration which accompanies expansion is, in reality, due to the consumption of heat in the performance of work by the expanding gas. Where no work is performed there is no absolute refrigeration."

A sufficient answer to both these would seem to be found in the fact that the "vacuum" spoken of is stated by Joule to have been obtained by means of an *air pump;* whence it appears that both vessels were *filled* with air; that in the exhausted receiver having already, during the process of exhaustion, absorbed and rendered latent, all the heat necessary for its expansion; and since we have already seen that the amount of latent heat in a given volume of air is almost entirely independent of density, we have no reason to look for any loss or gain of latent heat by the operation. The mixing of the two is quite as much a process of condensation as of rarefaction; in one receiver the air in expanding absorbs heat, whilst in the other the air being compressed gives out the heat which it had absorbed during the process of exhaustion—the two effects counterbalancing each other. The air, as a whole, just filled the two receivers at the beginning of the experiment, and it filled the same at the end; so that the effects of expansion and of condensation were completely eliminated: even more so than those of mechanical power, which Joule had especially in view when contriving this experiment.

Whilst the above seems to show that this experiment proved nothing as to the existence or non-existence of latent heat of expansion, any one who will read Joule's paper will probably be convinced that he never intended to claim that it did. He makes no allusion to any such question; and the limit which he gives of the sensitiveness of his thermometer shows that an amount of heat seventy-five times as great as that which could be expected to be rendered latent by doubling the volume of only two quarts of air, of any density, would be required to produce any appreciable change in the temperature of the 16½ lbs. of water through which, he tells us, it was distributed. He was dealing with mechanical power, and took care to have it in quantities large enough to be traced; his aim seeming to be, to solve the general question of their convertibility into heat, rather than to determine whether the results might, or might not, have been modified in some degree, by latent heat or other disturbing cause.

These considerations seem to justify us in concluding that the question of the existence of "latent heat of expansion" has not been experimentally decided in the negative, and that we may therefore proceed to inquire into its applicability to the explanation of meteoric phenomena,*

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* When this paper was read, I was not aware of the language of Joule himself on this subject. Part II. of the article "On the Thermal Effects of Fluids in Motion," by J. P. Joule and Prof. W. Thomson, (Transactions of the Royal Society, 1854, vol. 144 p. 337) speaking of the "Relations between the Heat evolved and the Work spent in Compressing

using the above value of x. But, the luminosity of meteors is usually attributed to atmospheric resistance. Kirkwood, in his Meteoric Astronomy (Philadelphia, 1867, p. 81), says: "Several hundred detonating meteors have been observed, and their average height at the instant of their first appearance has been found to exceed 90 miles. The great meteor of February 3d, 1856, seen at Brussels, Geneva, Paris, and elsewhere, was 150 miles high when first seen, and a few apparently well authenticated instances are known of a still greater elevation. We conclude, therefore, from the evidence afforded by meteoric phenomena, that the height of the atmosphere is certainly not *less* than 200 miles.

It might be supposed, however, that the resistance of the air at such altitudes would not develop a sufficient amount of heat to give meteorites their brilliant appearance. This question has been discussed by Joule, Thomson, Haidinger, and Reichenbach, and may now be regarded as definitively settled. When the velocity of a meteorite is known, the quantity of heat produced by its motion through air of a given density is readily determined. The temperature acquired is the equivalent of the force with which the atmospheric molecules are met by the moving body. This is about one degree F. for a velocity of 100 feet per second, and it varies directly as the square of the velocity. A velocity, therefore, of 30 miles in a second would produce a temperature of 2,500,000°. The weight of 5,280 cubic feet of air at the earth's surface is about 2,830,000 grains. This, consequently, is the weight of a column one mile in length, and whose base or cross section is one square foot. The weight of a column of the same dimensions at a height of 140 miles would be about

a Gas kept at a constant temperature," says, "This relation is not a relation of simple mechanical equivalence, as was supposed by Mayer. * * * The first attempt to determine the relation in question for the case of air established an approximate equivalence without deciding how close it might be, or the direction of the discrepancy, if any. Thus experiments "On the Changes of temperature produced by the Rarefaction and Condensation of Air," (Philosophical Magazine, May, 1845) showed an approximate agreement between the heat evolved by compressing air into a strong copper vessel under water, and the heat generated by an equal expenditure of work in stirring a liquid; and again conversely an approximate compensation of the cold of expansion when air in expanding spent all its work in stirring its own mass, by rushing through the narrow passage of a slightly opened stop-cock."

Whilst this language fully confirms my interpretation of Joule's experiment, the inference drawn by the authors from their subsequent experiments upon air forced through a "porous plug," composed of compressed cotton-wool or silk, is incompatible with the theory which I have advanced. They showed that when air was forced through such a plug, its temperature was lowered; and that the cooling effect was in proportion to the difference in the pressure of the air, on the two sides of the plug. For reasons previously stated by Prof. Thomson, (Transactions of the Royal Society of Edinburgh, vol. xx., 1851) they assumed that this cooling effect represented the amount of heat rendered latent by expansion; and hence concluded that this, also, varied directly as the difference of pressure.

It is, however, by no means self-evident, that the mechanical energy of the condensed air would be exactly balanced—neither more nor less—by the work done in overcoming the friction of the plug, and thus completely isolate the effects of latent heat of expansion This being only a theoretical deduction, ought not to have the weight of a direct result from experiment. Hence, perhaps, the omission of Tyndall and of Stewart to allude to it. If standard writers thus fail to recognize it as conclusive, we may fairly consider the subject as still open to discussion.

$\frac{1}{350000}$th of a grain. Hence, the heat acquired by a meteoric mass whose cross section is one square foot, in moving one mile, would be one grain raised $7\frac{1}{7}$ degrees, or one-fifth of a grain 2,500° in 70 miles. This temperature would undoubtedly be sufficient to render meteoric bodies brilliantly luminous.''

The above is a very clear statement of the resistance theory, which is the only one which seems to have met with general acceptance. But when we consider that the heat resulting from the collision of the atmospheric molecules with the surface of the meteorite, being developed at that surface, must be to a great extent absorbed by the meteorite ; and that, in the case supposed above, a body more than one foot in diameter had to travel seventy miles, to develop heat competent to raise one-fifth of a grain to a temperature less than that of melting iron, we must conclude that, at the height of 140 miles, the resistance theory utterly fails to account for any luminosity whatever.

In order to give some definite form to the discussion of the comparative effects of resistance and of latent heat in the production of meteoric luminosity, let us, with Kirkwood, suppose a globular meteor of one square foot section to enter the atmosphere with a velocity of thirty miles per second.

In traveling one mile, it will sweep a cylindrical space one mile long containing 5280 cubic feet, all the air in which will be compressed to a density at least as great as that of air at the surface of the earth, and be carried forward in front of the meteor. When in approaching the earth denser strata are reached, some portion of the air will of course be merely pushed aside and left behind, the air piled up in a conical mass in front of the meteor, dividing the atmosphere, just as the sharp bow of a vessel divides the water and thus diminishes the resistance ; but at great heights, if the velocity be great, this effect may be neglected.

Heat will be developed at the forward surface of the meteor, *firstly*—from the *resistance* of the air, which converts into heat a portion of the kinetic energy or motive power of the meteor ; its amount, at any given velocity, depending upon the *weight* of the air met ; *secondly*—from the release of *latent heat*, the amount of which depends only on the *bulk* of the air met.

The mere mention of the fact that the heating power of "resistance" depends upon the weight ; and that of "latent heat" upon the bulk of the air encountered, shows the great advantage which the latter has at extreme heights. "Latent heat" is at its maximum at the extreme upper limit of the atmosphere, where there is no appreciable weight of air to absorb the heat developed at the surface of the meteor. Its whole energies are therefore expended on the meteor, the surface layer of which may be so heated as to cause it to burst out in full splendor very soon after entering the atmosphere, and at a height where the weight of air encountered is so infinitesimally small that the effects of "resistance" are not perceptible : but no luminosity can be expected from either source until the heat developed is sufficient to produce incandescence, both in the surface layer of the meteor, and in its atmospheric envelope.

The comparative, as well as the absolute effects of "resistance" and of "latent heat" are illustrated in the accompanying tabular statement; from which we see that at the height of 103 miles, the latent heat is sufficient to raise the temperature of all the air met, six hundred millions of degrees; at sixty-eight miles, six hundred thousand degrees; at fifty-one miles, twenty thousand degrees; at thirty-four miles, six hundred

HEIGHT IN MILES.	WEIGHT IN GRAINS of air met in 1 mile by meteor 1 square foot cross section.	HEATING POWER OF LATENT HEAT in 1 grain of air at different heights.	HEATING POWER OF LATENT HEAT of air met in 1 mile at any considerable velocity.	HEATING POWER OF RESISTANCE of air met in 1 mile at a velocity of 30 miles per second.
nm.	$\frac{w}{2^n}$	$(2^n-1)x$	$\frac{2^n-1}{2^n}wx$	$M\frac{v^2}{64\frac{1}{3}\times772\times0.24}$
3.43	1491500	$\frac{2}{3}$	957543	3138839377500
17.15	93219	20	1855240	196175882730
34.30	2913	657	1913216	6128260320
51.45	91	21250	1915028	191508134
68.60	3	673200	1915084	6313454
85.75	$\frac{3}{32}$	21474800	1915086	197296
102.90	$\frac{3}{1024}$	687195000	1915086	6166
120.05	$\frac{3}{32768}$	21990233000	1915086	192
137.20	$\frac{3}{1048576}$	703687442000	1915086	6
154.35	$\frac{3}{33554432}$	22517998137000	1915086	$\frac{3}{16}$
171.50	$\frac{3}{1073741824}$	720575940380000	1915086	$\frac{3}{512}$
188.65	$\frac{3}{34359738368}$	23058558092137000	1915086	$\frac{3}{16384}$
205.80	$\frac{3}{1099511627776}$	737873858948446000	1915086	$\frac{3}{524288}$

Explanation :—M = mass of air. v = velocity of meteor in feet.
m = 3.43. nm = height in miles.
w = number of grains of air in 5,280 cubic feet at level of the sea = 2,983,000.
x = heat rendered latent by each addition of 1 vol. = 0.642° Fahr.

degrees (about the temperature of melting lead); whilst at the height of seventeen miles, the whole of the latent heat would be required to raise the temperature of the air only twenty degrees. From this it is evident that "latent heat" fails entirely as a source of luminosity at all heights below forty miles. On the other hand, whilst at great heights the effect of "resistance" is insignificant and altogether inadequate to the production of any splendor, its power at the height of forty, or even of fifty miles, seems almost unlimited. "Latent heat" and "resistance" together cover the whole field. Luminosity from "resistance" would commence at a height of eighty-five miles, more or less, according to velocity, and would increase rapidly with *decrease* of height, so that at the height of thirty-four miles it would be more than thirty thousand times as great as at eighty-five miles; whilst "latent heat" would cause the meteor to burst out in full splendor as soon as it had penetrated the atmosphere far enough to develop an amount of heat competent to vaporize its outer layer: and to disappear entirely, at a height of more than forty miles.

It is a significant fact, that very few meteors have been known to retain their luminosity below that point. Indeed, whilst some of the observed phenomena are such that "resistance" alone cannot afford any explanation whatever, they are all in perfect accord with the requirements of the "latent heat" theory. Hence we seem to be justified in concluding that "latent heat" is the *principal* source of meteoric luminosity.

The second column in the table gives the heating power of a unit of weight of air at different heights: showing, that one grain at the height of three and a-half miles, if compressed until its density equals that of air at the sea-level, will give out only enough heat to raise the temperature of one grain of air under constant volume about two-thirds of one degree; but that at the height of eighty-five miles the heat given out will suffice to raise the temperature of one grain twenty millions of degrees; at one hundred and thirty-seven miles, seven hundred thousand millions of degrees; whilst at the height of two hundred and five miles the number would exceed seven hundred thousand millions of millions.

This implies a condition of things somewhat similar to that suggested by Mr. Birks in his chapter on the "*Igneous condition of matter*,"* when he says, "There will thus, according to the present theory of the laws of matter, be more truth than has latterly been recognized, in the old arrangement of the four elements, which placed a fourth region of fire, above the solid, liquid, and gaseous constituents of our globe. In fact above the region where the air, though greatly rarefied, is still elastic, there must be a still higher stratum where elasticity has wholly ceased, and where the particles of matter, being very widely separated, condense around them the largest amount of ether. All sensible heat, in the

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* On Matter and Ether or the Secret Laws of Physical Change, by Thomas Rawson Birks, M. A., Cambridge, (England) 1862.

collision or oscillation of neighboring atoms of matter, will thus have disappeared : but latent heat, in the quantity of condensed ether or repulsive force, ready to be developed on the renewed approach of the atoms, will have reached its maximum, and may be capable of producing the most splendid igneous phenomena, like the northern lights or tropical thunder storms."

Quetelet, in view of the phenomena peculiar to the upper air, proposed to consider it as a distinct atmosphere, and says* "This upper atmosphere, favorable to the combustion and brilliance of shooting stars, would not necessarily be of the same nature and the same composition, as the lower atmosphere in which we live."

Sir John Herschel also seems to recognize something not unlike what I have suggested, when, in writing to Quetelet of shooting stars, in August, 1863, he says,† "As to their great elevation above the earth, it leads us to *suspect* the existence of a kind of atmosphere higher up than the aërial atmosphere, lighter, and, so to speak, more *igneous* than our own."

The train of reasoning which I have suggested leads to the conclusion that this "more igneous" condition commences at a height of forty or fifty miles, and extends to the utmost limit of the atmosphere ; its intensity increasing with the height in a geometric ratio—the outer shell of air being so completely saturated with heat, that, like a sponge filled with water, it responds to the slightest pressure. It is evident that this fiery envelope may prove a most efficient shield to protect us from the effects of collision with all sorts of fragmentary missiles which the earth may encounter in its journeys around the sun ; and the proof of its efficiency is found in the fact that of the immense number of meteors visible, only a very few have been known to reach the earth.

Fortunately, the enormous velocity—vastly exceeding that of the swiftest cannon ball--with which these missiles are hurled at us, usually causes their almost instantaneous destruction.

Were they simply dropped from $\frac{1}{2}$ or $\frac{1}{4}$ of the height, falling with the velocity due to the earth's attraction only, it is probable that every one of any appreciable weight, would reach the earth. Without this protecting envelope, we might well dread the effects of such a bombardment as was witnessed in Italy on the 27th November, 1873, when we encountered some of the dèbris of Biela's Comet, and when the number of meteors seen by the Italian Astronomers in the course of a few hours was estimated by them at near forty thousand.

The fact seems to be, that the planetary velocity with which a meteor enters our atmosphere, soon causes it to develop, by compressing the air before it—heat sufficient to vaporize its surface layer, and, to communicate to it the most dazzling brightness. Time not being allowed for the

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*Meteors, Aerolites, Storms and Atmospheric Phenomena. From the French of Zürcher and Margolle, by Wm. Lackland. New York, 1870, p. 229.

†Bulletins of the Royal Academy of Brussels, vol. XVI., p. 320.

heat to distribute itself through the body of the meteor, the whole of its effect is confined to the surface; extremely thin layers of which are, in succession, heated, rendered intensely incandescent, and vaporized, however refractory the material.

The black "crust," of the thickness of letter paper, with which the stony meteorites are coated, shows the limits for any one instant, of the melting process; and the fact, that beneath the crust there is no trace of the action of fire, is proof, both of the extreme intensity of the heat, and of its entirely superficial distribution.

Another disintegrating process may, perhaps, be mainly confined to the smaller meteorites and to the ordinary shooting stars, which are so completely dissipated that no trace of them reaches the earth. Although, in any individual layer, the three states—solid, liquid, and vapor—exist almost at the same instant, they must in reality succeed each other in the order named; so that there must always be a layer in which the material, although not melted, is so intensely heated as to exert an expansive energy tending to split the mass into fragments. The amount of decrepitation thus produced must, of course, depend upon the brittleness and other peculiarities of the meteor, as well as upon its velocity and upon the density of the air encountered; but the effect must be similar in character to that which takes place when coal being thrown upon the fire of a locomotive, minute fragments split off by the sudden expansion, are carried up the chimney and fall upon the car-roof in such numbers as to remind passengers of the rattle of a shower of hail.

It can scarcely be supposed that combustion has much to do with the splendor of meteors, or with their destruction, since these mainly occur at heights at which there is not air enough to maintain combustion to any considerable extent. Their disintegration must therefore be mainly effected by heat alone, unaided by chemical action.

Frequently, after the disappearance of a meteor of extraordinary splendor, a luminous train or cloud remains for a few seconds, sometimes for several minutes, and in some very rare instances they have remained visible for an hour or more. A remarkable example of this occurred on the 14th of November, 1868, when, shortly after midnight, a meteor appearing over Northeastern Pennsylvania, left a cloud which remained visible to observers at Washington and New Haven and at all intermediate points, for about three-quarters of an hour. According to Prof. Newton,* the observations indicated for this cloud "a real diameter of one mile, and a volume of a dozen or a score of cubic miles," and that whilst visible it moved about forty miles, showing an average velocity relatively to the earth of nearly a mile per minute. What was its velocity relatively to the air is not known. This cloud was, no doubt, the dèbris of the meteor, a cloud of meteoric dust, moving rapidly through the air, compressing the air before it; and, of course, if the above views be correct, developing heat and light, just as, on a grander scale, heat and light

* Silliman's Journal, vol. 47, p. 406.

had before been developed by the motion of the meteor itself. The intensity of the light must of course, diminish with the loss of relative velocity, and altogether cease whenever the cloud and the air are relatively at rest, or nearly so.*

The motion of a meteoric cloud, relatively to the air, may result either from its own momentum, from atmospheric currents, or from the diurnal rotation of the atmosphere, in which the meteor, of course, had not participated ; or from any or all of these causes combined, so that it must in almost all instances be very considerable.

The light of the aurora may perhaps in like manner be due to latent heat; for although rarefied air is a very good conductor, it probably offers resistance to the passage of electric currents sufficient to produce a momentary condensation quite competent to illuminate their paths.

It is evident that if the upper air be in the condition suggested, the track of *every* mechanical impulse, traversing it with considerable velocity, must become luminous.

This igneous condition of rarefied air necessarily implies a definite limit to the atmosphere of each member of the solar system : otherwise, meteors —being constantly subjected to the action of latent heat—would be luminous, not merely when within one or two hundred miles of the earth, but at all distances.

The depth of highly rarefied air which a meteor can traverse before becoming luminous, must of course, depend upon its velocity, temperature, and conducting power ; but the height at which their luminosity is seen to commence must afford some clue to the determination of the height to which the atmosphere extends.

The great comet of 1843, when in perihelion, Feb. 27, passed within sixty thousand miles of the surface of the sun, at a velocity of about 350 miles per second, and the next day was seen "as a brilliant body within less than two degrees of the sun."†

It was not seen again until about seven o'clock on the evening of March 7, when although the tail was a very conspicuous object, the brilliancy of the nucleus did not exceed that of a star of the third magnitude.

This change, so much greater than could reasonably be expected to result from increased distance from the sun, occasioned great surprise, and has not been satisfactorily accounted for.

Is it not possible that its splendor was temporarily increased by the latent heat developed during its passage through the solar atmosphere?

The great day-light meteor of Nov. 15, 1859, was seen at 9 o'clock in the morning, in full sunshine, by persons who were not within two hundred and fifty miles of any portion of its path, appearing so very bright that they thought it close at hand. Comparing the probable size

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*Prof. Newton remarks (Silliman's Journal, vol. 47, p. 407,) "What kind of matter it is which remains visible in the cold upper air for three-fourths of an hour until, by gradual dissipation, the light fades out, I leave for others to say."

†Kirkwood's Comets and Meteors, Phila., 1873, p. 17.

of the comet with that of the meteor, and remembering the prodigious velocity of the former, may we not well imagine that its collision with the highly attenuated upper atmosphere of the sun might develop latent heat sufficient to enable it to rival the sun itself in splendor?

Although much of the evidence presented in favor of the existence of "latent heat of expansion," and of its agency in the production of luminous phenomena, may be said to be circumstantial only,—I trust that it will be found sufficiently cumulative, and accordant throughout, to entitle it to examination.

PHILADELPHIA, MARCH 25TH, 1874.

ON THE PLAGOPTERINÆ AND THE ICHTHYOLOGY OF UTAH.

BY EDWARD D. COPE, A.M.

Read before the American Philosophical Society, March 20th, 1874.

The observations recorded below are based on the collections made by the naturalists attached to the United States Geological and Topographical Survey west of the 100th meridian, under direction of Lieutenant Geo. M. Wheeler, and are published by permission of that officer. To Dr. Henry C. Yarrow, in charge of the department of zoology, and to A. W. Henshaw, assistant, the survey is indebted for material more fully illustrating the character and distribution of the cold blooded vertebrata of the valleys of the Colorado River and of Utah than any heretofore brought together. As one of the results derived from a study of it, it appears that the basin of the Colorado River is the habitat of a small group of fishes of the family Cyprinidæ, which may be called the *Plagopterinæ*, which embraces three genera—*Plagopterus*, Cope; *Lepidomeda*, Cope; and *Meda*, Girard. The group differs from others of the family in the possession of two strong osseous rays of the dorsal fin, the posterior of which is let into a groove in the hinder face of the anterior without being coössified with it, thus constituting a compound defensive spine. The rays of the ventral fin, excepting the first and second, are similarly modified. The greater part of their length consists of an osseous dagger-shaped spine, with grooved posterior edge, which overlaps the border of the succeeding ray, when the fin, like a fan, is closed up. The articulated portion of the ray either emerges from the groove below the free acute apex of the spine, or appears as a continuation of the apex itself. It is worth observing that the only other instance of this ossification of the ventral rays is to be seen in the extinct family of the *Saurodontidæ* of the cretaceous period, the nearest approach among recent fishes being the internal spine in the ventral fin of *Amphacanthus*. The dentition and intestine of these fishes show them to be of carnivorous habits. Interest